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METEOROLOGY.—*A bundle of meteorological paradoxes.*[1] W. J. HUMPHREYS, Weather Bureau.

The scientific paradox is only an exception to some familiar but too inclusive generalization. It, therefore, has both the appeal of the riddle and the charm of surprise—the surprise, the instant the truth is seen, of a sudden and unexpected discovery—and thus affords the same sort of intellectual delight that I once knew a student of geometry to experience. The proposition, one of Euclid's best, was the Pythagorean, often carelessly called the *pons asinorum*. The boy in question was of that sturdy type that always insists on being "shown," and not understanding this proposition, flatly refused to accept it. A little coaching at the blackboard, however, soon got him past his initial troubles and so fixed his attention that as the truth flashed upon him with the final "therefore," he blurted out, in the ecstatic surprise of an Archimedes, and with the same oblivion to his surroundings, "Well, I'll be damned if it ain't so."

Whether the following paradoxes do or do not evoke such joyous acclamations as the one just quoted, they, nevertheless deserve to be concisely stated and fully explained for they express important facts of nature, unknown to, or, at most, but vaguely realized by the average person.

AIR PUSHED NORTH BLOWS EAST

This paradoxical behavior of the air is restricted, it should be said, to the northern hemisphere; but it seems just as contrarious

[1] Address of the retiring president of the Philosophical Society of Washington, delivered January 31, 1920.

on the other side of the equator, for there, pushed north it blows west, pushed south it blows east.

The push that causes the winds to blow is due to the existence of unequal amounts of air above a given level over adjacent regions—more at the place from which the air is pushed than at the place towards which it is pushed—and this in turn, usually, is due to the temperature differences, level for level, between the atmosphere at the two places. Obviously there tends to be, and, initially, actually is, a horizontal flow of the air (that is, a wind) at each level, in the direction of the most rapid horizontal decrease of pressure at that level. Such winds, however, frequently last so long (hours at least) that their directions are profoundly altered by a certain obscure factor, namely, the rotation of the earth—the secret of the above paradox—which is overlooked by almost everyone, and overlooked simply because its effect on the shooting of a marble, the pitching of a ball, and all the thousand other similar phenomena with which we are intimately familiar, is always negligible.

It is easy to demonstrate, as may be found in many books and articles, that an object moving in any horizontal direction tends so strongly to turn to the right north of the equator, and to the left south of it, as to exert a force, against a restraint preventing such deflection, given by the equation

$$f = 2\, m\omega v \sin\varphi,$$

in which m is the mass of the object, v its speed, φ its latitude, and ω the angular velocity of the earth's rotation.

Consider, then, the effect of applying a horizontal push of constant magnitude and constant geographic direction to a mass of air, m, and assume this air to be free from friction, as it very nearly is when appreciably above the surface. Let m, figure 1, be this mass of air, initially at rest with reference to the surface of the earth; let it be in the northern hemisphere, and let p be the push of constant magnitude and constant direction, north. Immediately the mass moves it begins to deflect from the north towards the east, and, owing to the curvature of its path, introduces a small centrifugal force. A little later p may be resolved, as shown, into two components, one normal and the

other tangential to the path of travel. The first, like the deflective force and the centrifugal force, has no effect on the speed, being at right angles to the direction of motion, while the second steadily increases the speed, which, in turn, increases the deflective force and the deviation towards the east. In the end, therefore, the component of p along the path reduces to zero, and the direction of travel becomes exactly east. Hence winds that are continuous for even a few hours always blow more or less closely along isobars, that is, at right angles to, and not in the direction of, the sustaining force—around centers of pressure minima and maxima and not directly towards or from them.

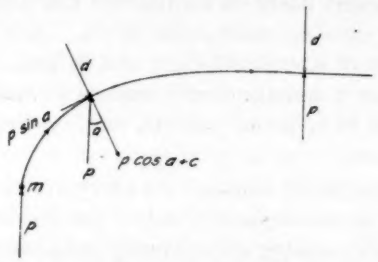

Fig. 1. • Diagram showing deflection of particle of air towards the east.

No matter, therefore, how paradoxical it may be, air pushed north does blow east (in the northern hemisphere), pushed east blows south, pushed south blows west, pushed west blows north; while in the southern hemisphere it blows exactly contrariwise.

RAIN DRIES THE AIR

As everyone knows, there is continuous and often rapid evaporation from practically all parts of the earth's surface. Nevertheless, the atmosphere as a whole never becomes even approximately saturated. Water, as just stated, is always evaporating into the air and thus constantly tending to saturate it; but, on the other hand, the air is forever being dried by the precipitation out of it of rain, snow and other forms of condensation. Whatever the temperature and relative humidity of a given mass of air at any place along its convectional route, the total of water vapor it then contains obviously is less, in general, than when

it left the surface of the earth by the amount of precipitation in the meantime abandoned by it. That is, on the average, air descends to the earth drier than it was when it ascended, and drier solely because of, and in proportion to, the amount of precipitation that fell out of it during its convectional journey. In short, as the paradox puts it, rain does dry the air—does prevent it from becoming and remaining everywhere reekingly and intolerably humid, as it otherwise would be.

MORE AIR GOES UP THAN EVER COMES DOWN

This is, perhaps, about as incredible a paradox as can be found, for it seems flatly to contravene the well known dictum that whatever goes up must come down. And indeed to make the explanation of it entirely clear and definite, it will be necessary to consider it independently under two heads: *a*, when the air is measured in terms of volume, and, *b*, when it is measured in terms of mass.

Measured in terms of volume.—As everyone knows, the vertical circulation of the atmosphere is only a gravitational phenomenon consisting of the sinking of relatively cold, and, therefore, also relatively dense air, and its consequent lifting or forcing up of adjacent air that happens to be comparatively warm and light. In short, contracted air descends and expanded air ascends (is buoyed up by the descending denser air). Hence, mass for mass, the *volume* of the ascending air is always larger than that of the descending air. The ratio between the actual ascending and descending volumes, however, or masses, may be anything, as illustrated by chimney circulation, in which the ascent is restricted to a comparatively small volume and mass moving rapidly, while the descent extends to a relatively large volume and mass settling slowly. On the average, though, considering both velocity of vertical movement and volume occupied, or velocity times volume, the atmosphere as a whole is always ascending, a fact not only interesting itself, but also of some importance to both the aeronaut and the aviator.

Measured in terms of mass.—Whatever the volume relations between ascending and descending air may be, it would seem

that at least the mass that goes up and the mass that eventually returns must certainly be the same. But, on the contrary, they indeed are far from it, for one of the important constituents of

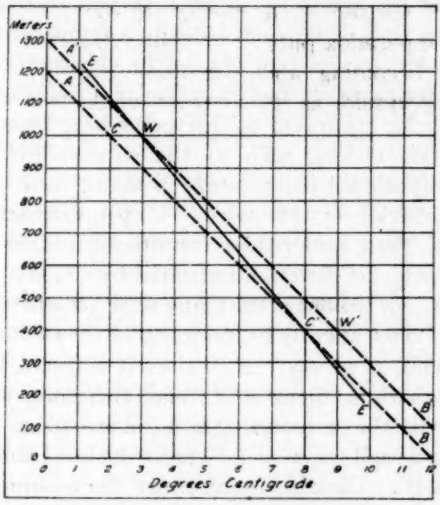

Fig. 2. Vertical temperature gradients of free air.

the atmosphere, water vapor, often amounting, in places, to 1 per cent, and occasionally to more than 2 per cent of the whole, invariably ascends as a gas, as a distinct part and parcel of the air; but descends, in great measure, not as a gas at all, not as any part whatever of the air, but as a liquid in the form of rain, or a solid, such as snow and hail.

Paradoxical, therefore, as it may be, a greater mass of *air* actually does go up—more by at least 20 million tons per second, the measure of world-wide precipitation—than ever comes down.

TO COOL AIR, HEAT IT

The air referred to in this seemingly absurd statement is not that topsy-turvy kind Alice might have found in Wonderland, but just that ordinary kind in which we have always lived; and

the phenomenon itself, however contrary to experience it may seem, one of great importance and almost continuous occurrence.

This paradoxical result is easy to explain with a diagram. To this end let AB and $A'B'$, figure 2, be two adiabatic gradients of the free air; that is, let each indicate a temperature change of 1° C. for every 100 meters change in elevation—the relation between the temperature and elevation of a rising or falling mass of air that during its travel neither gains heat from, nor loses it to, any outside object, such as the surrounding atmosphere. Let EE be any actual temperature gradient (nearly always less than the adiabatic), in this case 1° C. per 120 meters change of elevation. If, then, under these conditions, a mass of air having the temperature and elevation indicated by C', say, of the figure, be heated 1° C., or shifted in the figure to W', it will correspondingly expand and consequently be forced up by the surrounding denser air—will ascend, as we say. As it rises, it will cool, by expansion, along the adiabatic gradient $A'B'$, and, therefore, will come into equilibrium with the surrounding atmosphere where this gradient intersects the actual gradient EE, or at the level and temperature indicated by W. Clearly, then, under the assumed conditions, such as are very common in nature, a mass of air heated 1° C. rises 600 meters, and in so doing cools 6° C., or to a temperature 5° C. lower than it had before it was heated. Of course, the warm air does not rise strictly adiabatically, though probably very nearly so; but in so far as it actually does *lose* heat it comes to equilibrium at a correspondingly *lower* level and *warmer* temperature.

It is precisely this paradoxical process of cooling by heating, the heating being mainly at the surface, however, that leads to the formation of cumulus clouds and generates the familiar "heat" thunderstorm. In fact, it is quite possible to produce a cumulus cloud, and even a local shower, through the action of a large surface fire. It should be noted in this connection that though combustion adds much water vapor to the air, five ninths the weight of the fuel consumed even in the case of absolutely dry cellulose, nevertheless, the cumulus cloud over the fire is due essentially to the expansional or dynamical cooling of the ascending air.

TO WARM AIR, COOL IT

This paradox is the converse of the one just discussed, and is readily explained in much the same way. Referring again to figure 2, let a mass of free air having the altitude and temperature indicated by *W* in the figure, be cooled 1° C., or its position shifted to *C*. It will at once become denser than it was, follow the adiabatic gradient *AB* as it falls to lower levels, and, therefore, come to rest at the level and temperature indicated by *C'*, or at the intersection of the adiabatic gradient followed and the existing gradient. That is, as a result of the initial cooling of 1° C., the given mass of air will fall 600 meters and become 5° C. warmer than it was before it was first cooled. In so far, however, as the falling air *gains* heat from the surrounding warmer atmosphere, it will come to rest at a correspondingly *greater* elevation and *lower* temperature.

This paradoxical phenomenon of warming by cooling is very frequently and very prettily illustrated by the evening disappearance of small detached clouds, such as alto-cumuli, fracto-stratus, etc. As soon as the sun has set, these clouds and the air masses they fill cool more rapidly than does the clear atmosphere. They, therefore, fall to lower levels, warm up to higher temperatures than they originally had, and evaporate.

It will be interesting, in this connection, to note the logical effect of a certain ingenious, often proposed, and at least once experimentally tried, method of artificially inducing rainfall, namely, the liberal sprinkling of a cloud mass with liquid air. The result is, of course, an initial cooling of the cloud, followed, as above explained, by a much greater warming. Instead of rain being induced by this process, as its many inventors would confidently expect, the chilled cloud is certain to grow warmer and diminish in size, and, if considerably chilled may grow so much warmer as to disappear entirely. Indeed, this particular liquid air scheme is not a rain making process at all, but, on the contrary, a rain deterrent!

NOT AIR THAT IS HEATED, BUT AIR THAT IS NOT HEATED, IS THEREBY WARMED

This particular paradox may suggest the superiority of "absent treatment;" nevertheless, it is perfectly sound. Heated

air, as we know, is driven up by the surrounding denser air, and dynamically cooled, but the air that drives it does so by dropping to a lower level, where it is more or less compressed and correspondingly warmed. In other words, while the particular air that was heated rises and gets colder than it was initially, other air that was not heated at all falls lower and thus gets warmer. It is not the air that is heated, but air that is not heated, that gets warmer.

NOT AIR THAT IS CHILLED, BUT AIR THAT IS NOT CHILLED IS THEREBY COOLED

The explanation of this paradox is very similar to that of the one just given, and is equally simple. As the chilled air descends, certain other air is thereby raised and dynamically cooled. That is, while the particular air that was cooled descends and thus gets warmer than it was originally, other air that was not chilled at all is forced up, expands, and gets colder. It is not the air that is chilled (unless it happens to be on or near the surface where it cannot fall to a lower level) but air that is not chilled, that gets colder.

MIXING BRINGS THE AIR TO A NON-UNIFORM TEMPERATURE

To the laboratorian familiar with beakers and calorimeters; to the housewife skilled in the art of the cups and kettles; and to all the rest of us, nothing is more certain—nothing more in accord with daily experience—than that vigorous stirring establishes a uniform temperature throughout the agitated medium. And indeed this conclusion is quite correct in respect to the particular things we are likely to have in mind, but it does not apply to the open atmosphere. In fact if the temperature of the atmosphere were uniform through any considerable altitude, a complete stirring of it would immediately destroy this uniformity.

Let, then, the atmosphere, whatever its initial temperature distribution, be thoroughly mixed without the addition or subtraction of heat. This will bring it into such state (that of neutral equilibrium) that any portion of it on being adiabatically moved to a different place will, on arriving at that place, have the same temperature as the then adjacent air at the same level.

That is, it will have the same *potential* temperature throughout, or same actual temperature when subjected to the same pressure. The truth of the above statement is obvious from the fact that any temperature difference that might be developed by a transfer of the kind mentioned clearly could be reduced by further mixing.

But as a mass of this air is carried to higher levels it continuously expands against the diminishing pressure—diminished by the weight of the air passed through—thereby does work at the expense of its own heat energy and correspondingly cools to lower temperatures. The ratio of this cooling to increase of altitude evidently depends upon the nature of the gas and the change of pressure. In the case of our own atmosphere it is approximately 1° C. per 100 meters.

Although, therefore, stirring does bring an incompressible liquid to a uniform actual temperature, it brings the atmosphere only to a uniform potential temperature, or an actual temperature that is very non-uniform.

THE NEARER THE SUN THE COLDER THE AIR

The familiar fact that with increase of elevation and consequent approach (during the daytime) to the sun, the air nevertheless gets rapidly colder, at least through the first 10 kilometers, is very puzzling to the average person if he tries to explain it. Nor, indeed, is the explanation of this phenomenon quite so simple and obvious as we sometimes are asked to believe. Essentially, however, this temperature distribution depends on the following facts:

(1) The atmosphere, as we know from observation, is so diathermanous that half, roughly, of the effective radiation received from the sun, that is, half of the portion absorbed and not lost by reflection, goes directly to heating the surface of the earth. Consequently, it is this surface, where the energy absorption is concentrated, and not the atmosphere, through which absorption is diffused, that is most strongly heated by insolation. The heated surface in turn warms the air above it, partly by contact, and partly by the long wave-length radiation it emits, and

of which the atmosphere is far more absorptive than it is of the comparatively short wave-length solar radiation.

(2) Furthermore, and this is an equally vital part of the explanation, the lower atmosphere (below about 10 kilometers), under all ordinary conditions emits more radiant energy than it absorbs—the difference being supplied by conduction. It is these two phenomena, (*a*) the surface heating (warming below), and (*b*) the net loss of heat by radiation (cooling above), that together establish and maintain the vertical convections of the atmosphere under which, since the descending portions grow warmer through compression, and the ascending colder through expansion, the whole of the convective region is made to decrease in temperature with increase of elevation.

But since the coefficient of absorption of the air, as of other objects, changes but little if at all with the temperature, while its emissive power decreases rapidly as it grows colder, and since the intensity of the incident terrestrial (including atmospheric) radiation remains roughly constant up to an altitude of many kilometers, beyond the first 4 or 5, it follows that the upper limit of the convective region is not, as formerly supposed, the outermost extent of the atmosphere, but at that elevation (10 to 12 kilometers above sea-level) at which the temperature is so low ($-55°$ C. roughly) that the loss of heat by radiation is no longer in excess of, but now equal to, its gain by absorption. Beyond this level temperature does not decrease, or does so but slightly, with increase of elevation; nor would it so decrease (at least at anything like the present rate) beyond any level above the thin conducting surface layer, at which absorption and radiation became equal.

In short then, the air grows colder with elevation—the nearer the sun the colder the air—because (1) owing to its transparency to solar radiation it is heated mainly at the surface of the earth, and (2) because, at ordinary temperatures, it emits more radiation than it absorbs. These together so affect the density of the atmosphere as to induce vertical convections, and thereby to establish and maintain, throughout the region in which they are active, a rapid decrease of temperature with increase of elevation.

THE COLDEST AIR COVERS THE WARMEST EARTH

This paradoxical statement refers to the air of the stratosphere, with respect to which it is a well-known truth whatever the explanation may be.

It has doubtless been known since the dawn of intelligence that the top of a mountain is colder than the adjacent valleys, and that the highest among neighboring mountains has the coldest top. And for much more than a century, actually since November 30, 1784, it has been known from observations by balloonists, that the temperature of the free air also decreases with elevation, at least up to such altitudes as were attained by manned balloons. About the close of the last century, however, it became evident, through records obtained with sounding balloons, that in middle latitudes the temperature of the atmosphere continuously decreases, on the average, with increase of altitude up to only 10 or 12 kilometers above sea level, and then becomes substantially constant. Numerous subsequent records obtained at many places have shown the additional surprising fact that this isothermal region, or stratosphere as it is generally called, begins at a higher level, and is colder, over equatorial regions than over any other part of the world. Indeed, it seems to be 10° to 15° C. colder over the equator, where its average temperature is roughly −70° C., than, for instance, over the polar circles.

The temperature of the stratosphere appears to be determined chiefly by the intensity of the outgoing radiation from the earth and the intervening water vapor and hence it seems to follow that this radiation must be less intense over regions near the equator than over those of the middle and higher latitudes; a conclusion that merely shifts the burden of explanation from one paradox to another.

Obviously, the earth as a whole must emit, on the average, the same amount of radiant energy that it absorbs, but the distribution of the two certainly is different. In equatorial regions the upward movement of the atmosphere is so general and so strong that high haze, cirrus, and other types of clouds are exceedingly common, and the atmosphere necessarily humid

and, therefore, highly absorptive of earth radiation, to great altitudes, especially as anticyclones with their extensive regions of descending air are there unknown. Clearly, then, a large part of the radiation through the stratosphere of this region must come from the clouds and from water vapor that are very high and correspondingly cold, and therefore its intensity, it would seem, must be correspondingly feeble. The pent up heat below can find an outlet through horizontal circulation and radiation from lower and warmer levels in higher latitudes.

This, perhaps, is at least the partial explanation of why the minimum temperature of the stratosphere occurs over the tropical regions—why the coldest air covers the warmest earth.

AS THE DAYS GROW LONGER THE COLD GROWS STRONGER

This old proverb paradox expresses the well-known fact that our lowest temperatures do not occur at the time of the shortest days, or when the heat supply from the sun is least, but some time afterwards, when the days have grown longer and the supply of solar heat has increased. That is, over a considerable period, the air grows colder as the sun grows warmer. In the far interior of continents, especially if arid, this lag may not be more than a couple of weeks, but on many islands and along several coasts whose winds are prevailingly on-shore it is from one to two months.

To understand this phenomenon consider an object (representing the earth) suspended within a thermally opaque shell (assumed the source of incoming radiation) whose temperature is everywhere the same. For simplicity let the enclosed object be a "black body," that is, a full radiator and a perfect absorber. Let the absolute temperature of the shell be T and that of the enclosed object $T \pm t$. Under these conditions the rate of heat absorption by the suspended body is AKT^4, where A is its "equivalent" area and K the "black body" coefficient, while the rate of its emission is $AK(T \pm t)^4$. If, now, t is small in comparison with T, the rate of net gain or loss of heat by the enclosed object is $4AKT^3t$, approximately, and the ratio of its rate of temperature increase or decrease to the temperature difference, t, a constant inversely proportional to its heat capacity, assuming high conductivity. The limiting temperature T

would, therefore, never be fully attained, but forever approached asymptotically. Clearly, then, if the temperature of the shell were T and that of the enclosed object $T + t$, the latter would continue to grow colder through any finite time unless, and until some time after, the temperature of the shell were raised above the then temperature of the enclosed object.

The reasoning in this special case applies also to the normal daily temperature of the atmosphere (substantially that of the surface of the earth), provided, as will be assumed for the moment, that there is neither circulation nor any thermal effects due to water transformations—freezing, thawing, etc. It applies because the normal daily loss of heat through radiation to space by any given region is as though it were a full radiator at a certain temperature, and its normal daily gain of heat from the outside as though it were completely canopied by another full radiator also at a certain (generally different) temperature.

During the autumn, therefore, while there is still stored in the earth much of its summer gain of heat, and while the daily supply of energy from the sun is growing less and less per unit area, the average 24-hour temperature of the surface, and of the surface air, must be appreciably higher than that of equilibrium with the simultaneous incoming radiation—higher because of the additional supply of heat by conduction from its reservoir beneath the surface—and as the summer storage of heat in the earth is very large and also near the surface (but little penetrating beyond a depth of 5 or 6 meters) it is obvious, from the preliminary explanation above, that the minimum temperature cannot occur until some time after winter solstice, or when the days have again grown longer, and that the delay must depend on latitude, nature of surface, and a number of other factors.

The date of this minimum temperature is still further delayed, in many places, by the trend of warm ocean currents and the warmer surface drifts toward the higher latitudes, and by onshore winds. It is also affected, though probably but slightly, by the thermal effects of freezing, thawing, evaporation, and condensation.

The storage of heat in the earth while the days are long, its gradual delivery back to the surface while the daily supply from the sun is comparatively small; and the poleward drift of warm water at all seasons, together produce, as explained, the paradoxical result so admirably expressed by the proverb,

As the days grow longer
The cold grows stronger.

AS THE NIGHTS GROW LONGER THE HEAT GROWS STRONGER

It will be recognized at once that this paradox is only the counterpart of the one just discussed, and that it must also have substantially the same explanation.

As the days continue to grow longer after the time of minimum temperature, it is clear that from then on for several months the earth's gain of heat must be at a faster rate than its loss—that, in terms of the above explanatory hypothesis, the effective temperature of the shell is T and that of the enclosed object $T-t$. Under these conditions the earth, because of its large but finite heat capacity, must continue to slowly grow warmer until the incoming radiation has become less, that is, until the nights have grown perceptibly longer.

This lag, the lag of maximum temperature after the summer solstice, is also, like the lag of minimum temperature after the winter solstice, a function of location; generally least in the interior of continents and greatest on islands and near coasts whose prevailing winds are on-shore.

AS THE SUN DESCENDS THE TEMPERATURE ASCENDS

By this paradoxical expression it is only meant to state tersely the well-known fact that the warmest time of the day is not when the sun is on the meridian, or when insolation is greatest, but sometime in the afternoon when the sun has descended considerably from its maximum elevation. As everyone knows, night cooling reaches its greatest effect, on the average, just after daybreak. Hence, as the sun ascends the temperatures of the warming surface of the earth and of the lower air lag behind equilibrium with the incoming radiation, and continue to do so until the intensity of the insolation has passed well beyond

its maximum. That is, the temperature continues to rise for some time, generally 2 to 4 hours, after the sun has crossed the meridian—as the sun begins to descend from its highest point the temperature continues to ascend.

THE ABSOLUTE MAXIMUM DIURNAL INSOLATION (HEAT SUPPLY) IS AT THE SOUTH POLE

If I is the solar constant, or quantity of solar energy per minute per unit area, normal to the insolation at the limit of the atmosphere, then the total amount Q of solar energy per any consecutive 24 hours, per unit area of a horizontal surface, also at the limit of the atmosphere, is given by the equation

$$Q = \frac{1440}{\pi} I(\sin \varphi \sin \delta H + \cos \varphi \cos \delta \sin H)$$

in which φ is the latitude of the place in question, δ the declination of the sun at the time, and H the hour angle, in radians, between noon and sunrise, or sunset.

A great deal of interesting information is contained in this equation. The most interesting, perhaps, is the fact that if the value of Q for the equator at the time of the vernal equinox be represented by 1000, then that of the north pole at summer solstice is 1202, and that of the south pole at the corresponding solstice 1284; each being greater than the value of Q at that time for any other place in either hemisphere. The advantage in favor of the south pole is owing to the fact that the earth is then near perihelion, and, therefore, closer to the sun.

Not only does the absolute maximum diurnal insolation at the limit of the atmosphere occur at the south pole, but, owing to the great elevation of the south polar region, the dryness of its atmosphere and its comparative freedom from dust, so also does the corresponding maximum at the surface of the earth.

The days, however, of abundant insolation at the poles are comparatively few, nor is this insolation very effective in raising the temperature, owing to the high reflecting power and great heat of fusion of the always prevalent ice and snow. And so it happens that although for a time every year each pole receives more diurnal insolation than does any other place on the

earth, it is always cold; and the south pole, though having the greater maximum diurnal insolation, is the colder of the two, owing to its elevation and greater distance from open water.

THE HOTTER THE SUN THE COLDER THE EARTH

It is not yet universally conceded that this paradox, "the hotter the sun the colder the earth," really is true; but the evidence in favor of it is already very strong. It is known, for instance, that several extensive studies of the temperature records of the earth have all shown that on the average, it is a little colder during the years of sunspot maxima than during the years of sunspot minima. Furthermore, numerous careful measurements of the solar radiation made during the past dozen years or more, seem to compel the assumption, at least tentatively, that the effective temperature of the sun is greater during spot maxima than during spot minima. If, then, both these conclusions are true—if the temperature of the earth is lowest during spot maxima and the solar constant highest—it follows that the above paradox is also true.

But by what possible process can the earth get colder when the sun grows warmer? It has been suggested that the increase of the solar constant causes a corresponding increase in the atmospheric circulation, and, therefore, a decrease in the surface temperature, owing to the greater flow of cold air from the higher towards the lower latitudes. But the very great mixing of the convective portion of the atmosphere, and the consequent prevention of the formation of over- and under-flowing strata, seems to render this suggested explanation untenable.

The key to this paradox, may, perhaps, be found in the greater extent and density of the solar corona at the times of spot maxima than at the times of spot minima. The corona, since in large measure it is only so much dust about the sun, obviously must interfere with the passage of radiation through it, and to a far greater extent with the ultraviolet radiation than with the visible and infrared. Hence, during spot maxima, or when the solar atmosphere is dustiest, the solar energy must, it would seem, be poorest in ultraviolet radiation.

Now when cold dry oxygen, such as exists in the upper atmosphere, is acted upon by certain regions, at least, of the ultraviolet spectrum, some of it is converted into ozone, a substance known to be in the upper atmosphere to a far greater extent than in the lower. Hence when sunspots are most numerous the upper air should contain a minimum amount of ozone. But ozone is intensely absorptive of earth radiation and that too in the spectral region of its greatest intensity, and where water is least absorptive and carbon dioxide not at all. That is, at the time of spot maxima when the solar constant is (apparently) greatest, the earth's blanket of ozone is (presumably) least. Even, therefore, if the earth should be receiving an increased amount of heat at this time it might, nevertheless, grow slightly colder because of the coincident depletion of the heat-conserving blanket of ozone.

A greater general prevalence of cirrus and cirrus haze during spot maxima than during spot minima (indicated by certain observations) would also account for this paradox; because such clouds, owing to the size of their particles, shut out the short wave-length solar radiation more effectively than they shut in the long wave-length earth radiation. And perhaps these clouds really are generally most prevalent during spot maxima, and, therefore, at least a contributing factor to the cause of the corresponding temperature minima. At any rate the auroras are then most frequent, and they obviously generate nitrous oxide and other hygroscopic compounds which, because of their density, slowly fall to the cirrus level where they may produce cloud particles in an atmosphere whose humidity is much below that which otherwise would be essential to cloud formation.

The maximum, then, of the cirrus screen and the minimum of the ozone blanket, coincident with the highest temperature of the sun, may very well account for the above paradox—the hotter the sun the colder the earth.

THE COOLER THE SUN THE WARMER THE EARTH

This paradox is practically included in the one just discussed. It means that at times of sunspot minima, when the solar constant seems to be least, the average temperature of the earth is highest.

At the times of spot minima the solar atmosphere is clearest; the extreme ultraviolet radiation presumably, therefore, at a maximum; the upper atmosphere richest in ozone, and the earth most conservative of its heat, and, because of the minimum (if that be the case) of cirrus, also most receptive of solar radiation—so receptive and so conservative, perhaps, as to gain slightly in temperature despite the decrease in the heat supply.

THE SUN RISES BEFORE IT IS UP

This paradox about the sun rising before it is up is equally true of the moon and the stars, and is also one of the best known and easiest explained of all meteorological paradoxes.

Everyone is familiar with the fact that as light passes slantingly from one medium to another, as from air to glass, for instance, it does not continue on in the same straight line, but abruptly changes direction at the interface according to well-known laws. And the same thing is true of the rays of light that pass from space into and through the atmosphere of the earth; except that, in this case, as the density of the atmosphere gradually increases from zero at its outer boundary to a maximum at the surface of the earth, so too the change in direction of the entering light is equally gradual. The total change of direction by the time the surface of the earth is reached depends upon the wave-length, or color, of the light; the slope at which it enters, or zenith distance of the luminous object; the temperature and barometric pressure at the place of observation; the humidity; and several other minor factors. On the average, however, light from a star for instance, that appears to be 90° from the zenith, and, therefore, on the horizon—just rising, say—has been bent out of its original course by about 34′.5. That is, it comes into view (rises) while actually more than half a degree below the horizon. And as the angular diameter of the sun and the moon are each less than this horizon refraction, it follows that when the sky is sufficiently clear the whole of either luminary may be seen before even its topmost portion is up; that is, before it is geometrically above the horizon, or actually within 90° or less of the zenith.

THE SUN SETS AFTER IT IS DOWN

Since the virtual wave-length of a given radiation of celestial origin and, therefore, the value of its astronomical refraction is modified by the rotation of the earth, as are also certain scintillation phenomena, it follows that the above paradox is not identical with the one just explained. Nevertheless, as the spectra of the stars and other celestial objects all overreach the visible portion at each end it follows that the Doppler effect produces no appreciable alteration in the ensemble of the light from any one—merely a minute shift of its entire spectrum that can be detected only in the positions of definite lines.

But even this displacement of the spectral lines, due to the rotation of the earth, is far too small, roughly one three-hundredth the distance between the sodium D's, to affect detectably astronomical refraction. Hence as the sun, the moon, and the stars all rise before they are up, so too they must all set only after they have gone down.

ENTOMOLOGY.—*The generic name* Ceropales *Latreille* (*Hymenoptera*). S. A. ROHWER, Bureau of Entomology.

In 1915 Morice and Durrant (Trans. Ent. Soc. Lond., 1914, pp. 403, 406) synonymize the generic name *Ceropales* Latreille with the name *Arpactus* Jurine and propose an entirely new name for those interesting Psammocharid wasps which for more than a century have been known to students under the name *Ceropales*. This is only one of a number of most disconcerting nomenclatorial changes suggested by these authors because of their study of a discarded book review by Panzer. While the present author is of the opinion that from the nomenclatorial standpoint there is no way to disregard the Erlangen List, for it is under this name that the Panzerian book review is now commonly known, he does not believe that all of the changes suggested by Morice and Durrant are in accord with the various rules and opinions of the International Commission on Zoological Nomenclature. Since the receipt of the paper by Morice and

Durrant the writer has spent considerable time investigating the question and collecting the opinions of the various workers on Hymenoptera and has completed a statement of the case for presentation to the International Commission. In the meantime, and until it is possible to receive an opinion from the Commission, he has refrained from adopting any of the changes. The question presented by the genus *Ceropales* need not, however, wait for this decision as it is largely a zoological problem which is satisfactorily covered by existing rules and opinions. Furthermore certain new names have been used for species of this genus, and it seems desirable to review the case with the hope that by so doing unnecessary confusion will be avoided.

The answer rests largely on the principle of accepting genera for which the included species are not mentioned by name, but also partly on the principle of genotype selection for such genera. In regards to the first point if the codified rules on Zoological Nomenclature do not satisfactorily cover the point of accepting the generic names proposed by Latreille in 1796, (Prec. Car. Ins.) opinion 46 is very definite, and it seems to the author that the question answered under this opinion is entirely analogous to Latreille's work of 1796. In regard to the validity of the genera proposed in this work the writer believes that according to the International Code they are valid and must date from 1796, and that the type species must be chosen in accordance with the conditions specified in opinion 46.

Accepting the validity of the name *Ceropales* in the 1796 publication we still have the question of its genotype. If Morice and Durrant are correct the name would have to be transferred from the Psammocharid wasps to the Sphecoid wasps. But even here it seems to the writer they have not used the correct interpretation of opinion 46 or adhered to all the principles of genotype selection covered by the Code. The Code specifically says, "The meaning of the expression 'select a type' is to be rigidly construed. Mention of a species as an illustration or example of a genus does not constitute a selection of a type." Since it is as an example that Latreille referred a species to the genus *Ceropales* in 1802 he did not designate its type. Addi-

tional discussion of this point is, however, not necessary for this particular case because the species referred to *Ceropales* in 1802 cannot be the genotype in accordance with opinion 46. Briefly reviewed the case is as follows.

In 1796 Latreille described the genus *Ceropales* but assigned no species to it. The description he gave, while rather general and applying in most points equally well to certain genera in the families Psammocharidae and Sphegidae, has two characters (viz. the semi-circular labrum and long hind legs) which as far as the groups concerned are involved, apply only to the genus belonging to the Psammocharidae. In 1802 (Hist. Nat. Crus. Ins., 3: p. 339) Latreille again characterizes the genus *Ceropales* and this time cites the species *quinquencinctus* Fabricius and doubtfully "*campestris ?* F." It would appear thus far that *quinquencinctus* would have to be the type of *Ceropales*, but this species does not agree with the original description in the following characters: "Levre superieure demi-circulaire" and "Pattes posterieures longues dans quelques especes." Since *quinquencinctus* does not agree with the description it cannot be the genotype (opinion 46 says, "the genus contains all of the species of the world which come under the generic description as originally published") and in fact Latreille corrected his error in 1804 (Nouv. Dict. Nat. Hist., p. 180) and 1805 (Hist. Nat. Crus. Ins., 13: p. 283) and placing *quinquencinctus* in a new genus, *Gorytes*, and citing *maculata* Fabricius as an example of *Ceropales*. Added proof that Latreille desired in 1804 to correct the error of 1802 is found in the fact that in 1804 *Ceropales* and *Gorytes* are the only genera to which species are assigned. The species *maculata* agrees with the original generic description of *Ceropales* and could correctly be named as the type of the genus. This is exactly what Latreille did in 1810 (Cons. Gen. Crust. Ins., p. 437).

It is almost certain and partly confirmed by Latreille's remarks in 1802 (Hist. Nat. Crust. Ins., 3: p. 335) that as characterized and understood in 1796 the genus *Ceropales* contained species now placed in *Ceropales* and also species now referred to the genus *Gorytes* (*s. l.*) but inasmuch as the characte

of the description all apply to *Ceropales* (as now understood) and not to *Gorytes*, and because it is certain that Latreille intended the name for the groups of Psammocharid wasps it does not seem desirable or justifiable to go against the rules and opinions governing zoological nomenclature and change the interpretation of a name which has had standing for more than two generations. The following synonymy seems to the author to be correct:

Ceropales Latreille, 1796. Type.—*Evania maculata* Fabricius.

Agenioxenus Ashmead, 1902. Type.—(*Ceropales rufiventris* Walsh) *Ceropales robertsoni* Cresson.

Ceratopales Schulz, 1906 (an emendation which is accepted by Banks, Bul. Mus. Comp. Zool., 63: 1819, p. 248).

Hypsiceraeus Morice and Durrant, 1915. Type.—*Evania maculata* Fabricius.

Because of the controversy between Viereck and Ashmead (see Ent. N., 13: p. 275 and p. 318, 1902) concerning the generic name *Agenoxenus* a few words of explanation are necessary. In proposing the generic name *Agenioxenus* Ashmead definitely cited as the type *Ceropales rufiventris* Walsh. This species has been correctly synonymized with *Ceropales robertsoni* Cresson by Fox (Trans. Amer. Ent. Soc., 19: p. 57, 1892) and is a true *Ceropales*. The genus *Agenioxenus* is, therefore, a synonym of *Ceropales*. It so happens, however, that the probable specimen on which Ashmead founded his genus is a male of the variable *Batazonus interruptus* (Say.). Ashmead's statement that the specimen he had was probably a cotype of *C. rufiventris* is undoubtedly wrong as the specimen will not agree with the original description and bears only a name label in Ashmead's hand writing and the printed label "Through C. V. Riley." This case is covered by opinion 65.

RADIOTELEGRAPHY.—*Notes on beat reception.* L. W. Austin and W. F. Grimes, U. S. Naval Radio Research Laboratory.

Effect of Regeneration.—According to some authorities, the great sensitiveness of the oscillating tube is mostly due to its

regeneration, while, according to others, the sensitiveness is inherent in the beat method. With the autodyne the two factors are impossible to separate, but with the heterodyne this can be done. The experiment was made as follows: The regular laboratory long wave set with magnetic back coupling and without grid condenser was used, but with the back coupling much too loose for local oscillations. Oscillations were then produced by a separate heterodyne and audibilities taken on Nauen, the heterodyne coupling being adjusted to give the best signal. Then the back coupling of the regular set was increased to a point just before autodyne oscillations were set up and where with spark signals strong regeneration would be noted, but no increase in Nauen signals was observed even with retuning. The removal of the plate coil and bridging condenser from the receiving set, thus reducing it to a primitive audion, also had no effect.

Next, with a heterodyne coupling too loose to give the best signal, autodyne regeneration increased the strength of signal; that is, it seems that the back coupling of the receiving set regenerates the local oscillations so as to bring them up to optimum value, but has no observable effect on the strength of received signals. It may be that the resultant increase in sensitiveness due to regeneration and that due to oscillation is the sum rather than the product of the two, so that when they are added, the smaller increase due to regeneration is hidden by the great increase due to the oscillations.

Best Strength of Local Oscillations.—For the range 1–5000 audibility, the best signal is obtained with the same strength of local oscillations for any given circuit and wave-length. The optimum value varies with different vacuum tubes and with different ratios of inductance to capacity, increasing with increasing capacity.

Law of Response and Autodyne and Heterodyne.—In 1915, it was discovered (Journ. Wash. Acad., 6: 81. 1916), that the law of response of the oscillating tube (autodyne) within the limits of observational error, was linear, that is, that the telephone current was proportional to the first power of the radio frequency

received current in the antenna, instead of proportional to the square, as in the non-oscillating tube, the crystal, electrolytic, etc.

Recently experiments have been made which prove that the linear response law holds for tubes and also for crystal detectors when local oscillations are produced by a heterodyne. Dr. J. M. Miller has suggested that the linear law might not hold if the local oscillations were very weak, for example, if excited by a heterodyne with very loose coupling, but experiment shows that even here there is linear proportionality within the errors of observation.

Introduction of Resistance in the Oscillating Grid Circuit.—It was discovered in 1915 that if an oscillating vacuum tube (autodyne) be coupled to an antenna or loop, any amount of resistance can be introduced in the secondary circuit without reducing the strength of signal, provided the back coupling be strengthened so as to keep the local oscillations at the same strength. This resistance may amount to many thousand ohms, while a small fraction of this resistance, if placed in the antenna or loop circuit, will reduce the signal to silence. Recently it has been found that the same is approximately true with a plain vacuum tube, and even with a crystal detector, when excited to local oscillations by a heterodyne.

In the early experiments the phenomenon was ascribed to a negative resistance action, but this is hardly possible, since the grid circuit is out of tune with the signal, and of course the explanation could hardly be applied to the heterodyne or crystal. As a consequence of the above facts, it follows that with an oscillating receiving tube connected directly in a loop, the strength of received signal is independent of the loop resistance. This has been verified by experiment.

Effect of Varying the Capacity-Inductance Ratio in Oscillating Receiving Tube Circuits.—As the vacuum tube is a voltage operated detector of signals, it has been supposed that the sensibility will be greater, the greater the inductance capacity ratio in the grid circuit. It was reported in 1917 (Proc. I. R. E. 5: 245. 1917) that the sensibility was independent of this

ratio. Recent experiments made on Nauen with inductances varying from 2.5 mh. to 36 mh. again showed no change in sensibility, provided the local oscillations were kept at the optimum value. The telephone current appears to be proportional to the square root of the antenna watts.

Heterodyne, Autodyne and Sensitizing Circuit.—Commander A. H. Taylor and also Mr. Israel have found that for long waves the heterodyne with vacuum bulb is more sensitive than the autodyne. The explanation being that with the heterodyne the secondary can be set exactly on the signal wave-length, while with the autodyne it must be detuned to produce the beats.

In the Research Laboratory it has been found that there is no difference in sensibility, provided the optimum coupling between the primary and secondary is maintained in both cases. With a loose coupling as used by Commander A. H. Taylor, the signal with heterodyne is stronger unless a sensitizing circuit is used with the autodyne. The sensitizing circuit reported in 1915 (Proc. I. R. E., 4: 251. 1916) is a circuit consisting of an inductance and condenser so coupled to the secondary that the latter is given two free wave-lengths, one of which corresponds to the wave-length of the incoming signal, while the other gives the beat frequency. With the sensitizing circuit the autodyne is equal to or better than the heterodyne in sensibility at any coupling. Probably at the optimum main coupling the antenna itself acts to a certain extent as a sensitizing circuit.

While the sensitizing circuit has the advantage of being far simpler than the heterodyne, it is found that the heterodyne is less subject to interference.

Regeneration of Spark Signals.—It has been suggested that possibly the strengthening of spark signals observed with the autodyne when the back coupling is closed to a point just before the note is roughened by the local oscillations may be due to very weak oscillations not strong enough to effect the note. Experiments have, therefore, been made with weak heterodyne coupling to see if the same phenomenon could be found as with the autodyne. No strengthening in the spark signal was observed until the note was roughened.

ABSTRACTS

Authors of scientific papers are requested to see that abstracts, preferably prepared and signed by themselves, are forwarded promptly to the editors. The abstracts should conform in length and general style to those appearing in this issue.

TECHNOLOGY.—*Effects of glucose and salts on the wearing quality of sole leather.* P. L. WORMELEY, R. C. BOWKER, R. W. HART, L. M. WHITMORE and J. B. CHURCHILL. Bur. Standards Techn. Paper **138**. Pp. 38, pls. 2, figs. 23. 1919.

This paper contains a description of the methods used and the results obtained from the first of a series of tests to be made on this subject. Four brands of leather were tested; two tannages to which very small amounts of glucose and salts were added and two tannages to which larger amounts of these materials were added. The experimental work consisted of actual service tests on shoes, tests on a laboratory wearing machine, water absorption tests and complete chemical analyses of the original and worn leathers. Results are presented which show the variation in wear of the different leathers, the variation in wear of soles cut from different locations on the hide, the water absorption qualities of the leathers and the variation in chemical composition of the leathers in different parts of the hide for both the new and worn soles. From the results of the test there is no indication that the addition of glucose and salts is either beneficial or detrimental to the durability of the leather and it is conclusively shown that the greater part of the added glucose and salts was lost from the leather during wear while the other water-soluble materials appeared to be retained in the leather. P. L. W.

ANTHROPOLOGY.—*Native villages and village sites east of the Mississippi.* DAVID I. BUSHNELL, JR. Bur. Amer. Ethn. Bull. **69**. Pp. 111, pl. 17, figs. 12.

The material for this paper has been drawn mainly from the writings of early travelers, supplemented to a large extent by the results of modern archaeological explorations. A short description of the country and people, embodying the results of the latest researches, introduces the subject, and a bibliography of 71 titles concludes it.

J. R. SWANTON.

PROCEEDINGS OF THE ACADEMY AND AFFILIATED SOCIETIES

PHILOSOPHICAL SOCIETY OF WASHINGTON

822D MEETING

The 822d meeting was held at the Cosmos Club, October 25, 1919, and was called to order at 8.25 p.m. by President HUMPHREYS. Attendance, 52.

J. H. DELLINGER: *Principles of radio transmission and reception with antenna and coil aerials.*

The paper was illustrated by lantern slides and was discussed by Messrs. G. W. LITTLEHALES, W. J. HUMPHREYS, C. E. MENDENHALL, WILLIAM BOWIE, and General G. O. SQUIER. This paper has been abstracted in this JOURNAL (9: 641. Dec. 4, 1919).

F. L. MOHLER, PAUL D. FOOTE and H. F. STIMSON: *Ionization and resonance potentials for electrons in vapors of lead and calcium.* This paper was presented by Mr. FOOTE and was illustrated by lantern slides.

Measurements of electron currents in three-electrode vacuum tubes of the type previously described have been made in vapors of lead and calcium. The lead and calcium were boiled in porcelain tubes at temperatures of about 1000° and 900° C., respectively. Current-voltage curves in lead showed a resonance potential of 1.26 volts and an ionization potential of 7.93 volts. Applying the quantum relation $Ve = h\nu$ we find that 1.26 volts corresponds, within experimental error, to the frequency of a strong infrared spectrum line at $\lambda = 10{,}291$ Å, giving a theoretical value of the resonance potential 1.198 volts.

In calcium two resonance potentials were found at 1.90 volts and at 2.85 volts, of which the first is the more prominent. Ionization occurred at 6.01 volts. The ionization potential corresponds to the limit of the principal series 1.5 S, $\lambda = 2027$ Å, giving as the theoretical value $V = 6.081$ volts. The first resonance is determined by the line 1.5 $S-2\ p_2$, $\lambda = 6572.78$ Å, $V = 1.877$ volts. The second resonance corresponds to the line 1.5 $S-2\ P$, $\lambda = 4226.73$ Å, $V = 2.918$ volts.

The spectral relations of the first resonance potential and ionization potential are analogous to the relation found with other metals in this group. Work of other observers shows that both the lines 1.5 $S-2\ P$ and 1.5 $S-2p_2$ appear below the ionization potential in most metals of this group.

This paper was discussed by Mr. WHITE.

E. C. CRITTENDEN, *Corresponding Secretary.*

823D MEETING

The 823d meeting was held at the Cosmos Club, November 8, 1919, with President HUMPHREYS in the chair, and 55 persons present.

R. W. G. WYCKOFF: *The nature of the forces between atoms in solids.* This paper, which has since been published in this JOURNAL (9: 565. Nov. 19, 1919) was illustrated by lantern slides and discussed by Messrs. SOSMAN, WHITE, HUMPHREYS and BRAY.

H. L. CURTIS, R. C. DUNCAN and H. H. MOORE: *Methods of measuring ballistic phenomena on a battleship.* The paper was presented by Mr. CURTIS.

In 1917, apparatus was designed for obtaining the relative time of ejections of the three shells, when the three guns of one turret of a battleship were fired simultaneously. The method consisted in having a condenser charged to a high potential and circuits so arranged that when the shell emerged from the gun it short-circuited two wires which were stretched in front of the gun, thus completing the electric circuit and discharging the condenser through a point on a chronograph drum. The method was satisfactory although the spark which was produced was not as vigorous as had been expected. It was later shown that this was caused by the discharge taking place through the hot ionized gas which preceded the shell. This, however, did not appreciably affect the results, since approximately the same interval existed in all the guns.

It was soon found advisable to use a method which would be more flexible than the spark method outlined above. After careful consideration of the possible methods available, it was decided to use an oscillograph as a timing instrument. An oscillograph is simply a galvanometer of very high period which is critically damped, and which is arranged for photographing the movements of the mirror on a moving film. Three of these galvanometers or oscillograph elements are usually mounted in one instrument, all giving records on one film. To use this for timing various events, it is simply necessary to arrange an electric circuit in such a way that the events will produce a change in the electric circuit through the oscillograph element, thus causing a deflection of the mirror of the oscillograph element which is registered on the photographic film. It is also necessary to know the velocity of the film.

To obtain the velocity of the film, a tuning fork having small plates on the prongs is employed. When the tuning fork is at rest, a fine slit is cut through the two plates. The tuning fork is then mounted so that a beam of light passes through this slit and makes a line on the film when both the fork and the film are at rest. If now the fork vibrates and the film is in motion, lines will be produced on the film giving an accurate record of the velocity of the film. This method has already been described in a paper before the Society.[1]

The above method has been used to obtain with a single oscillograph

[1] Meeting of May 24, 1919, abstracted in this JOURNAL, 9: 642., Dec. 4, 1919.

element a number of time intervals between the closing of the firing circuit and the ejection of the shell from the gun. It is only necessary to so arrange the circuits that each event will produce a characteristic record on the film.

To obtain a record of a motion which is continuous, such as the recoil of the gun, a step-by-step method has been adopted. This consists of a series of contacts so arranged that at definite distances there will be a sudden change in the resistance of the circuit. This will produce a small but definite movement of the oscillograph element so that the time when this occurred is recorded on the oscillograph film. Then, from the known positions of the contacts, and the measured times, a curve can be plotted which will show the rate at which the movement took place.

This step-by-step method has the advantage that both the distances and the time can be accurately measured. Hence, at the points where the change in resistance takes place, the position and time are determined with a high degree of accuracy. If a curve is plotted, using time and position as coordinates, this curve is very accurate at all points, excepting where the motion is changing direction. If the total motion is known, the curve can be made accurate at this point also.

The above method has been used in the design of a recoil-meter. The steps are not the same throughout the entire length of recoil, but are shorter during the first part of the curve and longer during the latter portion. This increases the accuracy during the first part of recoil where a study of the motion of the gun is most important. Very satisfactory curves have been obtained.

The same principle has also been employed in the design of a kinemeter, which is an instrument for obtaining the motion of the gun in a direction perpendicular to its axis. This instrument is designed on the same principle as a seismograph, and has also given satisfactory results.

This paper was illustrated by lantern slides, and was discussed by Messrs. L. J. BRIGGS, WHITE, HAWKESWORTH and HULL.

The meeting adjourned at 10.10 and was followed by a social hour.

S. J. MAUCHLY, *Recording Secretary.*

WASHINGTON SOCIETY OF ENGINEERS

Fourteen meetings of the Society were held during the year 1919, as follows:

January 15, 1919: CHARLES R. MANN, Professor of Education and Director of Educational Research, *Engineering education.*

February 5, 1919: Major C. H. WEST, Chemical Warfare Service, U. S. Army, *Use of poisonous gases in modern warfare.*

February 19, 1919: Brigadier-General SAMUEL T. ANSELL, Acting Judge Advocate General of the Army, *Relationship of the engineer to the Army.*

March 5, 1919: MARSHALL O. LEIGHTON, Consulting Engineer, *Great Falls water power and its relation to the District of Columbia.*

March 19, 1919: CHARLES H. PAUL, Assistant Chief Engineer, Miami Conservancy District, Dayton, Ohio, *Flood prevention works in the Miami Valley.*

April 2, 1919: R. B. CANFIELD, Electrical Engineer, *Development of electric traction.* Motion picture, *King of the rails.*

April 16, 1919: Rear Admiral C. P. PLUNKETT, U. S. Navy, *The Fourteen-inch Naval Battery in France.*

May 7, 1919: Inspection trip to Camp A. A. Humphreys, Virginia.

October 1, 1919: C. T. CHENERY, Secretary of Conference on National Public Works, *A national department of public works.*

October 15, 1919: Hon. LOUIS BROWNLOW, President, Board of Commissioners, District of Columbia, *The proposed building zone plan for Washington, D. C.* Captain JOHN T. TALMAN, U. S. A., *Plans and progress of the Key Bridge.*

November 5, 1919: H. T. CORY, Consulting Engineer, U. S. Reclamation Service, *Reclamation of land in the South.*

November 19, 1919: General discussion of preliminary report of Engineering Council's Committee on Classification and Compensation of Engineers in the service of the Federal Government, led by JOHN C. HOYT, Hydraulic Engineer, U. S. Geological Survey. Members of all of the affiliated Societies of the ACADEMY were invited to attend this meeting.

December 3, 1919: Annual dinner. Speakers: Hon. EDWIN F. SWEET, Acting Secretary of Commerce; Hon. HENRY W. TEMPLE, Member of Congress; Dr. HOLLIS GODFREY, President, Drexel Institute; Mr. PHILIP N. MOORE, Consulting Engineer.

December 17, 1919: Annual meeting for the election of officers. The following officers were elected for the year 1920: *President*, E. C. BARNARD; *Vice-President*, R. L. FARIS; *Treasurer*, G. P. SPRINGER; *Secretary*, W. E. PARKER; *Directors*, E. F. WENDT, MORRIS HACKER, JOHN C. HOYT, OSCAR C. MERRILL, ANTHONY F. LUCAS, C. H. BIRDSEYE, J. S. CONWAY, F. W. ALBERT; *Committee on Membership*, R. L. FARIS, J. S. CONWAY, C. H. BIRDSEYE; *Committee on Meetings*, F. W. ALBERT, OSCAR C. MERRILL, R. H. DALGLEISH, B. P. LAMBERTON, JR., JAMES H. VAN WAGENEN.

W. E. PARKER, *Secretary.*

SCIENTIFIC NOTES AND NEWS

The Committee on Nomenclature, of the American Ornithologists' Union, consisting of Messrs. WITMER STONE (*Chairman*), editor of *The Auk*; JONATHAN DWIGHT, of the American Museum of Natural History; H. C. OBERHOLSER, of the Bio'ogical Survey and C. W. RICHMOND, of the National Museum, met in Washington on February 11–12, to consider the revision of the A. O. U. check-list of North American birds.

The Bureau of Biological Survey, U. S. Department of Agriculture, has begun a campaign, with State and local assistance, against pocket gophers in Arizona. The gopher destroys fruit trees and crops and also does considerable damage to irrigation ditches.

Several European starlings, all captured near the District of Columbia, have been presented recently to the National Zoological Park. This imported bird is becoming increasingly numerous around Washington.

A fundamental mercurial standard for testing sphygmomanometers, used for the measurement of blood pressure, has been constructed at the Bureau of Standards. There appears to be a great variation in the different types of blood-pressure gages now in use, and a fundamental study of their accuracy and design is needed.

A special camera for taking panoramic photographs of the interior of gun barrels which have been subjected to firing tests has been designed at the Bureau of Standards and is now under construction.

Recent investigations at the Bureau of Standards on wood columns from some of the temporary war buildings erected in Washington, made of green timber which has warped and cracked in seasoning, show that when warping and bending have occurred the strength is considerably reduced, but that cracking due to seasoning does not weaken the columns as long as they remain straight.

Mr. L. B. ALDRICH, of the Astrophysical Observatory, Smithsonian Institution, has built and partly tested a new "honeycomb pyranometer" for measuring nocturnal radiation. Tests so far completed are very promising. The flat-black surface of the ordinary pyranometer, which is not a physically perfect "black body" for the long wave-lengths radiated by the earth, is replaced by a surface made up of about 200 triangular cells, each about 3 mm. on a side by 13 mm. deep. A silvered mirror below effectively doubles the depth.

Dr. PAUL BARTSCH, of the National Museum, gave an illustrated lecture before the Nature Study Section of the Twentieth Century Club in February on "The ferns of the District of Columbia."

Mr. HARVEY BASSLER, who has held a temporary appointment on the U. S. Geological Survey since 1911 while a student at Johns Hopkins University, has joined the permanent staff of the Survey as assistant geologist, and has been engaged in field work in the Virgin River Oil Field, Utah.

Mr. HOYT S. GALE, geologist in charge of the section of non-metalliferous deposits of the Division of Geology, U. S. Geological Survey, who recently returned from Europe where he examined and reported on the potash deposits for the Geological Survey and Bureau of Mines, is on furlough for five months to make an examination of the oil fields of eastern Bolivia.

Mr. K. C. HEALD, geologist of the U. S. Geological Survey, is returning from Bolivia by way of the Amazon to the east coast of Brazil.

Mr. GEORGE LIVINGSTON, who has been acting chief of the Bureau of Markets, U. S. Department of Agriculture, since the resignation of Mr. CHARLES J. BRAND, was appointed chief of the Bureau on January 27. Mr. Livingston came to the Department in 1915 as assistant marketing specialist.

Sir OLIVER LODGE gave two lectures in Washington on February 10 and 11 on psychical subjects.

Rear Admiral ROBERT EDWIN PEARY, U. S. N. (Retired), died at his home, 1831 Wyoming Avenue, on February 20, 1920, in his sixty-fourth year. Admiral Peary was born at Cresson, Pennsylvania, May 6, 1856. He began his government service as a draftsman in the U. S. Coast and Geodetic Survey, and in 1881 became a lieutenant in the U. S. Navy. In 1886 he began the series of Arctic explorations which culminated in his attainment of the North Pole on April 6, 1909.

Dr. WALDEMAR T. SCHALLER has resigned as chemist in the Division of Physical and Chemical Research, U. S. Geological Survey, and has accepted a position with the Great Southern Sulphur Company, Incorporated, of New Orleans, Louisiana, operating at Orla, Texas.

Dr. CHARLES D. WALCOTT, Secretary of the Smithsonian Institution, has been elected a foreign member of the Kungliga Svenska Vetenskapsakademien (Royal Swedish Academy) of Stockholm.

ERRATUM.

On page 34, line 14 from the bottom of the page for 1718 read 1817.